do me the favour of reading my original note again, he will find that the object of my remarks was simply to test the truth of a definite assertion by Mr. Spencer that "the Second Law of Motion is an immediate corollary of the preconception of the exact quantitative relation between cause and effect." It was entirely beside my purpose to discuss the general psychological question of the formation of conceptions or preconceptions farther than as it is involved in the truth or otherwise of this particular assertion. Mr. Collier's note is therefore, as far as regards my remarks, entirely irrelevant and needs no other reply than to invite him, as Mr. Spencer declines to do so, to answer the simple and definite questions proposed by me as difficulties which Mr. Spencer is bound to answer, unless he is prepared to admit that he was wrong in the assertion on which I commented.

I have assumed throughout that Mr. Spencer means to assert that the Second Law of Motion is *involved in*, not merely that it *involves*, a particular preconception. And yet this latter is all that Mr. Collier asserts in the summing up of Mr. Spencer's argument, with which he concludes his note. If Mr. Collier truly represents Mr. Spencer, I can only say that, while the assertion may be admitted to be true, it certainly appears to me to be so trite as to be hardly worth formulating. The whole question turns on the distinction between "involving" and "being involved in," which I suppose Mr. Spencer and Mr. Collier would regard as an important one, though it is difficult in some cases to make out distinctly from their language and their line of argument which they mean to imply.

Passing in conclusion beyond the particular issue to which I have hitherto confined myself, I would remark that to my mind all that Mr. Spencer's and Mr. Collier's illustrations prove is that, while unconscious experiences (whether individual or inherited) may give rise to certain general, but (except in the very simplest cases) vague, preconceptions, it is only when these preconceptions are wedded to consciously-made observation or experiment that they cease to be barren generalities and give birth to the fruitful laws of Physical Science. To a mathematician, at any rate, it is almost ridiculous to observe how little either Mr. Spencer or Mr. Collier seem to realise the great gap between the indefinite observation that two things always increase and decrease simultaneously, and the definite conclusion that they are proportional to one another. For example, it is hardly a parody of Mr. Collier's remarks to say :—"A child discovers that the greater the angle between his legs the greater the distance between his feet, an experience which implicates the notion of proportionality between the angle of a triangle and its opposite side ;" a preconception, as it appears to me, with just as good a basis as that whose formation Mr. Collier illustrates, but one which, as I need hardly add, is soon corrected by a conscious study of geometry or by actual measurement.

Harrow, May 25 ROBT. B. HAYWARD

MR. COLLIER'S letter, NATURE, vol. x. p. 43, is even more astonishing than anything that Mr. Spencer has written. A mathematician who reads it feels something like Alice behind the looking-glass ; and perhaps behind the looking-glass it may be "a question pertaining to the psychological basis of inductive logic," with which mathematicians, as such, have nothing to do. But in this world, this side the looking-glass, in which forces are measured and effects are measured, Mr. Collier's letter is very perplexing.

For example, after giving several instances in which a greater force produces a greater effect, Mr. Collier proceeds: "The experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced : and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved."

Does Mr. Collier know what proportionality means? Does any one of the experiments indicated prove that where effort is doubled the result is *doubled?* The child pulls his boat by a string through the water ; if he pulls twice as hard does he pull it *twice* as fast?

It seems to me that the people on the other side of the looking-glass think perfect quantitative equivalence (however measured) means the same as proportionality ; and are willing to raise first the general result of experience, that greater forces produce greater effects, into an axiom of exact quantitative equivalence (without troubling themselves to consider how quantity is to be estimated), and then to accept Newton's Second Law as an instance of this quantitative equivalence, without showing any connection between quantitative equivalence and direct proportionality in that instance or in any other.

A SENIOR WRANGLER

Ocean Circulation

MR. CROLL will doubtless be of opinion that as my "theories" show such an utter ignorance of "even the elements of physics and mechanics," I can employ my time much better in acquiring some knowledge of those sciences, than in continuing to discuss the subject with him.

I shall be glad to be allowed to state to the readers of NATURE, as I have to those of the *Philosophical Magazine* (May), other grounds on which I must decline to prolong this discussion.

1. Mr. Croll has charged me (Phil. Mag. for March, p. 177, note) with a serious misstatement in regard to the mean annual rate of the Gulf Stream, which he affirms to be *nearly double* what I have represented it. Now *my* statement was avowedly based on the *average of the whole year's observed rates ;* whilst Mr. Croll has taken as the basis of *his* the arithmetical *mean* between the maximum and the minimum. It has been said in disparagement of statistics that "anything can be proved by figures ;" and Mr. Croll, who is nothing if not a statistician, seems to me to justify the imputation, for the adoption of his method would make the *average* number of children of a marriage to be at least *ten !*

2. Mr. Croll, in asserting that I have left out of consideration "the fact that the sea is salter in intertropical than in polar regions, and that this circumstance, so far as it goes, must tend to neutralise the difference of temperature," has only exhibited his own ignorance of a very important fact of Ocean Physics—the *low* salinity of equatorial surface-water ; which was ascertained in Kotzebue's voyage fifty years ago, has been confirmed by many later series of observations, has been repeatedly cited in text-books, and has been adduced by myself as an indication that polar water is continually ascending from the bottom to the surface under the equator. But further, not only has this fact been confirmed by the *Challenger* observations, but so remarkable an accordance has been shown by them to exist between the low specific gravity of equatorial *surface*-water and that of equatorial *bottom*-water, as strongly to indicate that, as the latter is certainly polar, the former is so also. It suited Mr. Croll's purpose, however, with these observations before him, completely to ignore them, and to state as fact what is the precise contrary of facts.

3. According to Mr. Croll and his anonymous authority, the Astronomer Royal must be unfamiliar with even "the elements of physics and mechanics ;" for, speaking from the chair of the Royal Society in 1872, he explicitly expressed his acceptance of the doctrine I advocate, as "certain in theory and supported by observation." The eminent meteorologist, Prof. Mohn, of Christiania, also, who expressed to me in writing last year his acceptance of it, must be equally ill-informed ; as, too, must be Dr. Meyer of Kiel, who has been engaged for four or five years past in the investigation of the physics of the Baltic, the North Sea, and their connecting channels, and who has satisfied himself so completely of the power of small differences of specific gravity to put large bodies of water in motion. I have *nowhere said* that no eminent physicist shares Mr. Croll's objections ; though I have not myself met with such a one.

I regret to have been forced, by the personal attacks in which Mr. Croll has latterly thought fit to indulge, thus to retort upon him. Henceforth I shall not consider myself called upon to take any notice of assertions and arguments which I do not find to exert the least influence on the opinions of the eminent scientific men with whom it is my privilege to associate.

WILLIAM B. CARPENTER

Glacial Period

IN answering Mr. Bonney's letter in NATURE, vol. x. p. 44, I shall confine myself to the consideration of his second objection to my theory, as the precise southern limit of the glacial action is not of present importance, and the height of the Scandinavian sea-beaches is irrelevant to the inquiry.

Mr. Tiddemann, in an admirable paper On the glaciation of North Lancashire (Quart. Journ. Geol. Soc., vol. xxviii. p. 471), has mapped out the course of the ice as shown by scratched rocks, lines of transported boulders, carriage southwards of local

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drift, and direction of overturned edges of shaly strata, and proved that it did not flow down the valleys to the westward, but passed across them and the ranges separating them, from the north to the south. On the other side of the Irish Sea the Rev. Mr. Close and others have shown that there also the ice did not move down the valleys, but flowed along the coast southwards. The ice-scratches still preserved on the rocks prove that the configuration of the land was nearly the same then as now, and no explanation has ever been offered of this southerly movement of the ice, excepting that it was prevented from flowing down the natural slope of the land by the whole of the Irish Sea having been at the time filled with ice up to a height of about 2,000 ft. above the present sea-level. The Isle of Man, that lay in the path of this great ice-stream, is glaciated from top to bottom, and it must have been completely buried in ice. It is to the action of this great agent that I ascribe the pushing up of sand and shells over south Lancashire, Cheshire, and North Wales on one side, and Wexford and around Dublin on the other, of the Irish Sea, where the course of the ice southward was obstructed by the narrowing of the channel.

So far from the movement of this great mass of ice being deflected or warded off by local glaciers, we have seen that in north Lancashire it was not affected by them; and long ago Prof. Ramsay proved that the glaciation of Anglesea and the west of Caernarvonshire had not radiated from the high land, but that the ice had come from the north and brought with it numerous boulders from the mountains of Cumberland.

As to the possibility of ice, pushed forward by higher accumulations behind it, thrusting before it loose sand and shells up to higher levels, I may remark that there are many proofs that it possesses this power. In the Isle of Man blocks of granite have been pushed up 600 feet above the level of their source. Mr. Tiddemann has also shown that as the ice moved across the valleys down one side and up the other, it thrust over the edges of the strata. On the other side of the great English watershed, Mr. Dakyns has shown that the ice when ascending the slope of a valley opposed to its course swept before it all the drift lying on the surface, pushing it over to the other side of the range.

Mr. Bonney would be more likely to damage my theory if, instead of protesting against it, he could explain some of the many difficulties that beset that of submergence. Where was the shore of that mythical sea under which England nearly to the Thames is supposed to have been submerged? How is it that not a single undisturbed bed of glacial shells has been found, that nearly all are broken to pieces, that many fragments of Cyprina exhibit glacial scratchings, and that not a single instance has been recorded of the two valves of a lamellibranch having been found together? Was there no friendly cliff or cavern able to preserve a single shell from the ruthless second advance of the ice? Mr. James Geikie finds the fragile bones of water-rats and frogs in his "inter-glacial beds," and uninjured land and fresh-water shells occur in abundance; but not one marine shell has been found in the uplands that does not show proof of having been transported, by being broken, worn, or scratched.

Since my first letter was sent to NATURE, Prof. Ramsay has drawn my attention to Mr. Croll's theory, that the glacial shells of Holderness had been pushed up by ice over the land out of the German Ocean. From other papers of the same geologist, I gather that he does not dispute the submergence of much of England and Scotland during part of the glacial period, and has indeed proposed a theory to account for it. So far as I know I stand alone at present in the opinion that neither during nor since the glacial epoch has there been any submergence of a great part of the British Isles beneath the waters of the ocean, nor can I expect that a theory so contrary to what has been taught for many years by English geologists will obtain a ready acceptance. THOMAS BELT

Ealing, May 22

Uncompensated Chronometers

WITH reference to the employment of an uncompensated chronometer to indicate the mean temperature of an accompanying compensated chronometer during a long journey, with a view to the application of the proper correction for temperature, Prof. G. Forbes remarks (NATURE, vol. x. p. 50):—

"This method is quite new, and has not been tested by any nations except the Russians."

Permit me to direct attention to the following passage in the "Report on the Coast Survey," which I extract from p. 66 of the Proceedings of the American Association for the Advancement of Science, Springfield meeting, August 1859. The "Cambridge" referred to is Cambridge, Massachusetts.

"The difference of longitude between Cambridge and Liverpool has also been determined by means of large numbers of chronometers carried repeatedly between the two stations on the Cunard steamships. These chronometric expeditions, in the words of Mr. W. C. Bond, director of the Harvard Observatory, 'for the magnitude and completeness of their equipments, have not been equalled by any of the similar undertakings of European Governments. Even the *Expedition chronometrique* of Struve was on a scale much less extensive.' The voyages were continued through a number of successive years. The first great special expedition took place in 1849, and the most recent in 1855. In the latter the effect of temperature on the rate of the chronometers formed a subject of special investigation. For each instrument the effect of temperature on its rate was ascertained by experiment, and the average temperature during each trip was kept account of by means of a thermometric chronometer, constructed like the others, but with individual balance, so that its daily rate was affected by six seconds for a change in temperature of 1° Fahr. Fifty-two chronometers were employed in this expedition, and were transported six times between Cambridge and Liverpool."

The "Greenwich Observations" for many years past (fifteen at least) contain a record of the indications of a "chronometrical thermometer" accompanying the chronometers on trial for purchase by the Admiralty; and on p. 2 of "Rates of Chronometers" in the volume of Observations for 1871 are these words:—

"The chronometrical thermometer differs from an ordinary chronometer only in the construction of the balance, the positions of the metals forming the compensating rims being reversed. By this arrangement the effect of temperature is much magnified." J. D. EVERETT

Malone Road, Belfast, May 22

Photographic Irradiation

IN NATURE, vol. x. p. 29, the article on the coming Transit of Venus makes mention of photographic irradiation as having "been found by Lord Lindsay and Mr. A. C. Ranyard to be mainly due to the reflection of light from the back of the glass plate. It can be almost entirely avoided," Mr. Forbes goes on to say, "by wetting the back of the plate and placing black paper against it." This subject has been investigated, explained, and the above remedy suggested years ago by practical photographers. In 1867 I used the plates of the Liverpool Dry Plate Company, then sent out with the backs painted red to prevent irradiation.

But even this is not a complete preventive. Colouring the film, as suggested by Mr. Carey Lea of Philadelphia and Henry Cooper, a well-known English amateur, is a much more effectual means, though at a loss of sensitiveness; but the most complete (where the dry emulsion process is available) is to allow the collodion to be acted on by a large excess of nitrate of silver for a considerable time and then to convert this into bromide of silver by addition of ammonium bromide. The result is that the film has a dull opaque character like unglazed porcelain, and not only stops the light more completely than an ordinary collodion film, but remedies another cause of irradiation—the molecular reflection in the film itself.

Two years ago I tested plates prepared in this way on the most difficult subjects (not astronomical) and found the halation much less than by any other means except a deep red tint in the film. W. J. STILLMAN

Hay Fever

REFERRING to the recent article in (NATURE, vol. x. p. 26) upon hay fever, I can give my own experience as having suffered from the complaint for some years past, mainly in the months of May and June. My most severe attacks have been in the house in early morning. I am, however, near hay-fields, and a tramp, by way of experiment, through one of these has more than once satisfied me of the efficacy of the hay pollen in vastly increasing the troublesome symptoms.

The treatment I have used to myself has consisted of rather strong doses of quinine taken internally, and externally a piece of linen rag dipped in strong camphorated spirit and placed upon the nose and also partly over the nostrils.

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